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Goodbye Washington Consensus, Hello Washington Confusion? A Review of the World Bank's *Economic Growth in the 1990s: Learning from a Decade of Reform*

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Proponents and critics alike agree that the policies spawned by the Washington Consensus have not produced the desired results. The debate now is not over whether the Washington Consensus is dead or alive, but over what will replace it. An important marker in this intellectual terrain is the World Bank's Economic Growth in the 1990s: Learning from a Decade of Reform (2005). *With its emphasis on humility, policy diversity, selective and modest reforms, and experimentation, this is a rather extraordinary document demonstrating the extent to which the thinking of the development policy community has been transformed over the years. But there are other competing perspectives as well. One (trumpeted elsewhere in Washington) puts faith on extensive institutional reform, and another (exemplified by the U.N. Millennium Report) puts faith on foreign aid. Sorting intelligently among these diverse perspectives requires an explicitly diagnostic approach that recognizes that the binding constraints on growth differ from setting to setting.*

1. *Introduction*

Life used to be relatively simple for the peddlers of policy advice in the tropics. Observing the endless list of policy follies to which poor nations had succumbed, any well-trained and well-intentioned economist could feel justified in uttering the obvious truths of the profession: get your macro balances in order, take the state out of business, give markets free rein. "Stabilize, privatize, and liberalize" became the mantra of a generation of technocrats who cut their teeth in the developing world and of the political leaders they counseled.

Codified in John Williamson's (1990) well-known Washington Consensus, this advice inspired a wave of reforms in Latin America

* Harvard University. I am grateful to Roger Gordon for his encouragement and comments; to Ricardo Hausmann, Lant Pritchett, and John Williamson for their reactions; and to Roberto Zagha for the many insights he has shared with me over the last few years. John Williamson reminded me that my title is far from original, having been used in almost identical form by Moises Naim (1999). In its present form, the title also makes allusion to the classic paper by Carlos Diaz-Alejandro (1985).

and Sub-Saharan Africa that fundamentally transformed the policy landscape in these developing areas. With the fall of the Berlin Wall and the collapse of the Soviet Union, former socialist countries similarly made a bold leap toward markets. There was more privatization, deregulation, and trade liberalization in Latin America and Eastern Europe than probably anywhere else at any point in economic history. In Sub-Saharan Africa, governments moved with less conviction and speed, but there too a substantial portion of the new policy agenda was adopted: state marketing boards were dismantled, inflation reduced, trade opened up, and significant amounts of privatization undertaken.[1]

Such was the enthusiasm for reform in many of these countries that Williamson's original list of do's and don'ts came to look remarkably tame and innocuous by comparison. In particular, financial liberalization and opening up to international capital flows went much farther than what Williamson had anticipated (or thought prudent) from the vantage point of the late 1980s. Williamson's (2000) protestations notwithstanding, the reform agenda eventually came to be perceived, at least by its critics, as an overtly ideological effort to impose "neoliberalism" and "market fundamentalism" on developing nations.

The one thing that is generally agreed on about the consequences of these reforms is that things have not quite worked out the way they were intended. Even their most ardent supporters now concede that growth has been below expectations in Latin America (and the "transition crisis" deeper and more sustained than expected in former socialist economies). Not only were success stories in Sub-Saharan Africa few and far in between, but the market-oriented reforms of the 1990s proved ill-suited to deal with the growing public health emergency in which the continent became embroiled. The critics, meanwhile, feel that the disappointing outcomes have vindicated their concerns about the inappropriateness of the standard reform agenda. While the lessons drawn by proponents and skeptics differ, it is fair to say that nobody really believes in the Washington Consensus anymore.[2] The question now is not whether the Washington Consensus is dead or alive; it is what will replace it.

The World Bank's *Economic Growth in the 1990s: Learning from a Decade of Reform* (2005, henceforth *Learning from Reform*) is one of a spate of recent attempts at making sense of the facts of the last decade and a half, and probably the most intelligent. In fact, it is a rather extraordinary document insofar as it shows how far we have come from the original Washington Consensus. There are no confident assertions here of what works and what doesn't—and no blueprints for policymakers to adopt. The emphasis is on the need for humility, for policy diversity, for selective and modest reforms, and for experimentation. "The central message of this volume," Gobind Nankani, the World Bank vice-president who oversaw the effort, writes in the preface of the book, "is that there is no unique universal set of rules . . . [W]e need to get away from formulae and the search for elusive 'best practices'. . ." (p. xiii).[3] Occasionally, the reader has to remind himself that the book he is holding in his hands is not some

[1] To cite just one example, fifty percent or more of the state-owned enterprises were divested during the 1990s in the Central African Republic, Cote d'Ivoire, Gambia, Ghana, Guinea-Bissau, Kenya, Mali, Tanzania, Togo, Uganda, and Zambia (John Nellis 2003). On the extent of trade reform in Africa, see Vinaye D. Ancharaz (2003).

[2] In a book edited with Pedro-Pablo Kuczynski in 2003, John Williamson laid out an expanded reform agenda, emphasizing crisis-proofing of economies, "second-generation" reforms, and policies addressing inequality and social issues (Kuczynski and Williamson 2003).

[3] Roberto Zagha led the team that prepared the report. Members of the team were J. Edgardo Campos, James Hanson, Ann Harrison, Philip Keefer, Ioannis Kessides, Sarwar Lateef, Peter Montiel, Lant Pritchett, S. Ramachandran, Luis Serven, Oleksiy Shvets, and Helena Tang.

radical manifesto, but a report prepared by the seat of orthodoxy in the universe of development policy.

2. *The Record*

Here is how *Learning from Reform* summarizes the surprises of the 1990s. First, there was an unexpectedly deep and prolonged collapse in output in countries making the transition from communism to market economies. More than a decade into the transition, many countries had still not caught up to their 1990 levels of output. Second, Sub-Saharan Africa failed to take off, despite significant policy reform, improvements in the political and external environments, and continued foreign aid. The successes were few—with Uganda, Tanzania, and Mozambique the most commonly cited instances—and remained fragile more than a decade later. Third, there were frequent and painful financial crises in Latin America, East Asia, Russia, and Turkey. Most had remained unpredicted by financial markets and economists until capital flows started to reverse very suddenly. Fourth, the Latin American recovery in the first half of the 1990s proved short-lived. The 1990s as a whole saw less growth in Latin America in per capita GDP than in 1950–80, despite the dismantling of the state-led, populist, and protectionist policy regimes of the region. Finally, Argentina, the poster boy of the Latin American economic revolution, came crashing down in 2002 as its currency board proved unsustainable in the wake of Brazil's devaluation in January 1999.

Significantly, the period since 1990 was *not* a disaster for economic development. Quite to the contrary. From the standpoint of global poverty, the last two decades have proved the most favorable that the world has ever experienced. Rapid economic growth in China, India, and a few other Asian countries has resulted in an *absolute* reduction in the number of people living in extreme poverty.[4] The paradox is that that was unexpected too! China and India increased their reliance on market forces, of course, but their policies remained highly unconventional. With high levels of trade protection, lack of privatization, extensive industrial policies, and lax fiscal and financial policies through the 1990s, these two economies hardly looked like exemplars of the Washington Consensus. Indeed, had they been dismal failures instead of the successes they turned out to be, they would have arguably presented stronger evidence in support of Washington Consensus policies.[5]

Along with this telling, if anecdotal, evidence has come a more skeptical reading of the cross-national relationship between policy reform and economic growth. Characteristically, it is the World Bank itself that has been prone to make grandiose claims on the impact of policy reform. In one particularly egregious instance cited by William Easterly (2005), Paul Collier and David Dollar (2001) argued that policy reform of the conventional type could cut world poverty by half. Work by Easterly (2005) and Francisco Rodríguez (2005) show that the data do not support such claims. The evidence that macroeconomic policies, price distortions, financial policies, and trade openness have predictable, robust, and systematic effects on national growth rates is quite weak—except possibly in the extremes. Humongous fiscal deficits or autarkic trade policies can stifle economic growth, but moderate amounts of each are associated with widely varying economic outcomes.[6]

[4] According to World Bank estimates, there were roughly 400 million fewer people living below the $1 a day poverty line in 2001 compared to two decades earlier (Chen and Ravallion 2004).

[5] See Dani Rodrik (2005a) for an interpretative survey of recent growth experience.

[6] See also Rodrik (2005b) for a general methodological critique of growth regressions with policy variables on the right-hand side.

The question is how to interpret this recent experience, and how to turn the interpretation into concrete policy advice. Here *Learning from Reform* makes some valuable progress. I summarize some of the main conclusions below, emphasizing those that depart most strongly from the earlier approach.

3. *The Interpretation*

One of the insights of *Learning from Reform* is that the conventional package of reforms was too obsessed with deadweight-loss triangles and reaping the efficiency gains from eliminating them, and did not pay enough attention to stimulating the dynamic forces that lie behind the growth process. Seeking efficiency gains does not amount to a growth strategy. Although the report does not quite put it in this way, what I think the authors have in mind is that market or government failures that affect accumulation or productivity change are much more costly, and hence more deserving of policy attention, than distortions that simply affect static resource allocation. They may also be harder to identify. Focusing on the latter instead of the former results in small benefits, and could even turn out to be counterproductive when policy makers face a political budget constraint (more reform in one area means less reform in another).

A second conclusion is that the broad *objectives* of economic reform—namely market-oriented incentives, macroeconomic stability, and outward orientation—do not translate into unique set of policy actions. In the words of the Report, "The principles of . . .'macroeconomic stability, domestic liberalization, and openness' have been interpreted narrowly to mean 'minimize fiscal deficits, minimize inflation, minimize tariffs, maximize privatization, maximize liberalization of finance,' with the assumption that the more of these changes the better, at all times and in all places—overlooking the fact that these expedients are just *some* of the ways in which these principles can be implemented" (p. 11, emphasis in the original). The authors go on to point out that each of these ends can be achieved in a number of ways. For example, trade openness can be achieved through lower import tariffs, but also through duty drawbacks, export subsidies, special economic zones, export processing zones, and so on. This renunciation of standard "best practice" in World Bank policy advice is quite remarkable, and must not have come without a significant internal fight.

Third, different contexts require different solutions to solving common problems. Enhancing private investment incentives may require improving the security of property rights in one country but enhancing the financial sector in another. Technological catch-up may call for better or worse patent protection, depending on the level of development. This explains why countries that are growing—the report cites Bangladesh, Botswana, Chile, China, Egypt, India, Lao PDR, Mauritius, Sri Lanka, Tunisia, and Vietnam—have such diverse policy configurations, and why attempts to copy successful policy reforms in another country often end up in failure.

Fourth, *Learning from Reform* argues that there has been a tendency to exaggerate the advantages of rules over discretion in government behavior. Rules were meant to discipline the malfeasance of governments. But it turns out that "government discretion cannot be bypassed" (p. 14). Argentina's currency board, which removed monetary policy from the hands of the government, worked well when the binding constraint was lack of credibility, but led to disastrous outcomes when the binding constraint became an overvalued currency. There is no alternative to improving the processes of decisionmaking (better checks and balances, better guiding principles, better implementation) such that discretion leads to better outcomes.

Finally, reform efforts need to be selective and focus on the *binding constraints* on economic growth rather than take a laundry-list

approach à la Washington Consensus. While there is no foolproof method of identifying these constraints, common sense and economic analysis can help (see below). When investment is constrained by poor property rights, improving financial intermediation will not help. When it is constrained by high cost of capital, improving institutional quality will hardly work. Experimentation and learning about the nature of the binding constraints, and the changes therein, are therefore an integral part of the reform process. Even though countries may face situations in which many constraints need to be addressed simultaneously, the report judges these situations to be rare: "In most cases, countries can deal with constraints sequentially, a few at a time" (p. 16).

Taking these conclusions at face value, what they entail is nothing less than a radical rethink of development strategies. Of course, it would be naïve to think that the World Bank's practice will therefore change overnight. There is little evidence that operational work at the Bank has internalized these lessons to any significant extent.[7] And, as I will discuss below, there are contending interpretations of what has gone wrong and how to move forward. But the mere fact that such views have been put forward in an official World Bank publication is indicative of the changing nature of the debate and of the space that is opening up within orthodox circles for alternative visions of development policy.

4. *The Alternatives I: Institutions*

Around the same time that the World Bank was grappling with the lessons of the 1990s, its sister institution across the street, the International Monetary Fund (IMF), put out a document that focused on much the same issues in the context of Latin America (Anoop Singh et. al. 2005). This is an equally remarkable document which shows that in Washington there is anything but consensus these days. The IMF report starts from the same basic premise—growth has been disappointing—but its basic argument could not be more different. According to its authors, the problem was not with the approach taken to reform, but that it did not go deep and far enough. Using the report's own words, "reforms were uneven and remained incomplete" (p. xiv). "More progress was made," the IMF report claims, "with measures that had low up-front costs, such as privatization, relative to reforms that promised greater long-term benefits, such as improving macroeconomic and labor market institutions, and strengthening legal and judicial systems" (p. xiv). The same diagnosis is expressed succinctly in the title of one of Anne Krueger's speeches on policy reform: "Meant Well, Tried Little, Failed Much" (Krueger 2004). From this perspective, the failures have to be chalked up to too little reform of the kind that Washington has advocated all along and not to the nature of these reforms itself.[8] The policy implication that follows is simple: do more of the same, and do it well.

Several key ideas underpin this interpretation of the evidence. First, political leaders may have had the talk, but they didn't quite have the walk: their commitment to genuine reform was often "skin-deep" and there was "lack of follow-through" (Krueger 2004). Second, and more fundamentally, even committed reformers stopped well short of undertaking the full gamut of institutional changes needed to create well-functioning market economies. Regulatory and supervisory institutions in product and financial

[7] Along with Ricardo Hausmann and the lead author of the World Bank report, Roberto Zagha, I have been involved in an effort to bring some of these implications to bear on the country operational work at the Bank. One thing we have discovered is how difficult it is to wean the Bank's country economists away from the Washington-Consensus, laundry-list, best-practice approach to reform.

[8] But even within the IMF, there are divergent views. The IMF's Evaluation Office (nominally independent and headed until recently by a distinguished outsider, Montek Ahluwahlia, but staffed largely by IMF economists) has produced reports that often reach different conclusions.

TABLE 1
The Augmented Washington Consensus

Original Washington Consensus	"Augmented" Washington Consensus the previous 10 items, plus:
1. Fiscal discipline	11. Corporate governance
2. Reorientation of public expenditures	12. Anti-corruption
3. Tax reform	13. Flexible labor markets
4. Financial liberalization	14. WTO agreements
5. Unified and competitive exchange rates	15. Financial codes and standards
6. Trade liberalization	16. "Prudent" capital-account opening
7. Openness to DFI	17. Non-intermediate exchange rate regimes
8. Privatization	18. Independent central banks/inflation targeting
9. Deregulation	19. Social safety nets
10. Secure Property Rights	20. Targeted poverty reduction

markets proved too weak. Poor governance and corruption remained a problem. Courts and the judiciary were ineffective. And labor market institutions were not sufficiently "flexible."

Of course this second point, about the lack of emphasis on institutional reform, is itself an implicit repudiation of the original version of the Washington Consensus, insofar as the latter did not feature institutional reform of the type that Krueger and the IMF have in mind in their interpretation of the 1990s. Most of the items in Williamson's original list were relatively simple policy changes (liberalize trade, eliminate currency overvaluation, reduce fiscal deficits, and so on) that did not require deep-seated institutional changes. Williamson did include "property rights" in his list, but that was the last item on the list and came almost as an afterthought.

What has become clearer to practitioners of the Washington Consensus over time is that the standard policy reforms did not produce lasting effects if the background institutional conditions were poor. Sound policies needed to be embedded in solid institutions. Moreover, there were significant complementarities across different areas of reform. Trade liberalization would not work if fiscal institutions were not in place to make up for lost trade revenue, capital markets did not allocate finance to expanding sectors, customs officials were not competent and honest enough, labor-market institutions did not work properly to reduce transitional unemployment, and so on. The upshot is that the original Washington Consensus has been augmented by a long list of so-called "second-generation" reforms that are heavily institutional in nature. The precise enumeration of these requisite institutional reforms depends on who is talking and when, and often the list seems to extend to whatever it is that the reformers may *not* have had a chance to do—which is one of the problems that I will discuss below. Nonetheless, one possible rendition is shown in table 1, where I have listed ten second-generation reforms to maintain symmetry with the original Washington Consensus.

This focus on institutions has also received a strong boost from the (largely unrelated) rediscovery of institutions as a driver of long-term economic performance in the empirical literature on economic growth. In particular, Daron Acemoglu, Simon Johnson, and James A. Robinson's (2001) important work drove home the point that the security of

property rights has been historically perhaps the single most important determinant of why some countries grew rich and others remained poor. Going one step further, Easterly and Ross Levine (2003) showed that policies (i.e., trade openness, inflation, and exchange rate overvaluation) do not exert any independent effect on long-term economic performance once the quality of domestic institutions is included in the regression. Often, this work has taken a form that may be called "institutions fundamentalism"—to relate it to (and distinguish it from) the earlier wave of "market fundamentalism." Getting the institutions right is the mantra of the former, just as getting prices right was the mantra of the latter. The Augmented Washington Consensus derives its academic support largely from this work on the primacy of institutions.[9, 10]

Taken to its logical conclusion, the focus on institutions has potentially debilitating side effects for policy reformers. Institutions are by their very nature deeply embedded in society. If growth indeed requires major institutional transformation—in the areas of rule of law, property rights protection, governance, and so on—how can we not be pessimistic about the prospects for growth in poor countries? After all, such institutional changes typically happen very rarely—perhaps in the aftermath of war, civil wars, revolutions, and other major political upheavals. The cleanest cases that link institutional change to growth performance occur indeed at such historical junctures: consider for example the split between East and West Germany, or of North and South Korea. But what are poor countries that do not want to go through such upheavals to do?

[9] A mea culpa here: My article on "Institutions Rule" (Rodrik, Arvind Subramanian, and Francesco Trebbi 2004) is frequently seen as being in the frontline of institutions fundamentalism (although there are important caveats in the second half of the paper).

[10] The most serious challenge to institutions fundamentalism has been launched by Edward L. Glaeser, Rafael La Porta, Florencio Lopez-de-Silanes, and Andrei Shleifer (2004) who find the empirical approach in the institutions-cause-income literature flawed and think it is human capital (and dictators) that cause growth.

Learning from Reform pays lip service to the importance of institutions, but to its credit it steers clear from too much institutions determinism. That is wise because the Augmented Washington Consensus' focus on institutional change proves to be largely a dead-end upon closer look. There are two major reasons for this, which I summarize here.

First, the cross-national literature has been unable to establish a strong causal link between any particular design feature of institutions and economic growth. We know that growth happens when investors feel secure, but we have no idea what specific institutional blueprints will make them feel more secure in a given context. The literature gives us no hint as to what the right levers are. Institutional function does not uniquely determine institutional form. If you think this is splitting hairs, just compare the experience of Russia and China in the mid-1990s. China was able to elicit inordinate amounts of private investment under a system of public ownership (township and village enterprises), something that Russia failed to do under Western-style private ownership. Presumably this was because investors felt more secure when they were allied with local governments with residual claims on the stream of profits than when they had to entrust their assets to private contracts that would have to be enforced by incompetent and corrupt courts. Whatever the underlying reason, China's experience demonstrates how common goals (protection of property rights) can sometimes be achieved under divergent rules. This is a theme that *Learning from Reform* loudly trumpets.

Second, we should not forget that Acemoglu, Johnson, and Robinson (2001) work and other related research focused on *long-term* economic performance. The typical dependent variable in this line of literature is the *level* of income in some recent year, not the rate of economic growth over a particular period. When institutional indicators are

introduced in *growth* regressions, the results are much weaker and less robust. Empirical work focusing on transitions into and out of growth has found little evidence that large-scale institutional transformations play a role (Hausmann, Pritchett, and Rodrik 2005; Benjamin F. Jones and Benjamin A. Olken 2005). To take two important examples, China embarked on rapid growth in the late 1970s with changes in its system of incentives that were marginal in nature (and certainly with no ownership reform or significant change in its trade regime early on), and India's transition to high growth in the early 1980s was preceded (or accompanied) by no identifiable institutional changes. These and other experiences suggest that a policy maker interested in igniting economic growth may be better served by targeting the most binding constraints on economic growth—where the bang for the reform buck is greatest—than by investing scarce political and administrative capital on ambitious institutional reforms. Of course, institutional reform will be needed eventually to sustain economic growth. But it may be easier and more effective to do that when the economy is already growing and its costs can be spread over time.

In the limit, the obsession with comprehensive institutional reform leads to a policy agenda that is hopelessly ambitious and virtually impossible to fulfill. Telling poor countries in Africa or Latin America that they have to set their sights on the best-practice institutions of the United States or Sweden is like telling them that the only way to develop is to become developed—hardly useful policy advice! Furthermore, there is something inherently unfalsifiable about this advice. So open-ended is the agenda that even the most ambitious institutional reform efforts can be faulted ex post for having left something out. So you reformed institutions in trade, property rights, and macro but still did not grow? Well, it must be that you did not reform labor-market institutions. You did that too but still did not grow? Well, the problem must be with lack of safety nets and inadequate social insurance. You reformed those with little effect? Obviously the problem was that your political system was unable to generate sufficient credibility, lock-in, and legitimacy for the reforms. In the end, it is always the advisee who falls short, and never the advisor who is proved wrong.

5. *The Alternatives II: Foreign Aid*

Yet another vision of reform strategy is offered by the U.N. Millennium Project (2005), led by Jeffrey Sachs. This vision is no less holistic than that of the institutions fundamentalists, although the elements of the package and the weight placed on each differ. The U.N. Project calls for a comprehensive and simultaneous increase in "public investments, capacity building, domestic resource mobilization, and official development assistance," while providing "a framework for strengthening governance, promoting human rights, engaging civil society, and promoting the private sector" (p. xx). But it also abounds in concrete details of what can and should be done. Some of the "quick-win actions" it proposes include free distribution of bed nets against malaria, ending user fees for primary education and essential health services, expansion of school meals programs in hunger zones, and replenishment of soil nutrients on smallholder agriculture through subsidized or free distribution of chemical fertilizers.

The U.N. Millennium Project views current levels of foreign aid to be a significant constraint on the achievement of global poverty reduction. Hence it calls for a significant increase in aid—a doubling of annual official development assistance to $135 billion in 2006, rising to $195 billion by 2015—to finance public investments in human capital and infrastructure and to develop the technologies needed to transform health and agriculture in poor societies. Sachs and his collaborators exhibit a certain

impatience with those who argue that the real constraint is poor institutions and weak governance, and that large aid flows are more likely to disappear in the pockets of corrupt officialdom than to foster development. They argue that many of the poorest countries of the world (e.g., Benin, Mali, Senegal) have in fact made significant strides in improving their economic and political institutions, and that in any case the investments in human capital that they advocate would likely foster better institutions as well. In their view, the obsession with governance is often just an excuse for rich countries not doing more to help poor nations.

The theory underlying the U.N. Millenium Project's view of the world is that low-income countries in Africa (and possibly elsewhere) are stuck in a low-level equilibrium, a "poverty trap" (Sachs et al. 2004). The neoclassical production function assumes that the marginal product of capital is high at low levels of development (when the economy has low levels of capital). But if there are some increasing returns to scale (e.g., setting up a modern factory requires a minimum investment to be made), complementarities (e.g., running a modern factory needs an adequate supply of educated workers), or negative feedback effects (e.g., an increase in incomes raises population growth), the marginal return to capital is initially low rather than high. Small increments to capital yield very little fruit, and the economy can have multiple steady states, one of which involves a poverty trap. Since it does not pay to invest, households do not save and the economy remains poor. This very old idea (going back at least to Paul N. Rosenstein-Rodan (1943) and Richard R. Nelson (1956)) can be used to justify a "big push"—i.e., a large-scale, simultaneous effort to raise the capital stock (public, private, human) to levels where the neoclassical forces of convergence begin to operate and the economy breaks free of the poverty trap.

Several questions are raised by this take on African poverty. First, what do we make of the fact that historically few low income countries have embarked on high growth in this big-push fashion or through the infusion of large amounts of foreign aid? As Sachs's critics love to point out, there has not been a shortage of foreign aid in Africa, and some of the most rapidly growing countries of the past have done so without relying much on Western aid. Sachs and his collaborators counter that Africa is special because it suffers from high transport costs, low-productivity agriculture, a very heavy disease burden, adverse geopolitics, and slow diffusion of technology from abroad (Sachs et al. 2004, pp. 130–31), all of which make the region particularly prone to a poverty trap. But couldn't one have said much the same of Vietnam, a war-torn, impoverished country facing economic sanctions from the United States, which took off in the late 1980s even though it did not receive much aid from Western nations until the mid-1990s?

Or what do we make of the fact that economic growth is actually not uncommon among Sub-Saharan African nations themselves? The theory of poverty traps suggests that these countries are stuck in low-level equilibria from which they find it very hard to extricate themselves. The reality seems to be somewhat different. Most African countries have shown themselves capable of producing economic growth over nontrivial time horizons. A telling statistic produced by Jones and Olken (2005) is that three-quarters of Sub-Saharan African countries have grown fast enough to experience some convergence with U.S. income levels over at least one ten-year period since 1950. Similarly, in Hausmann, Pritchett, and Rodrik (2005), where we studied growth accelerations since the 1950s, we found such accelerations to be quite frequent in low-income countries, including among those in Africa. In fact, growth accelerations turned out to be more common in low-income countries than in middle- or high-income countries, in line with the neoclassical growth model. The trouble seems to be not that poor African countries are unable to

grow, but that their growth spurts eventually fizzle out. This suggests a rather different remedy, one that focuses in the short run on selectively removing binding constraints on growth (which may well differ from country to country), and in the medium- to longer-run on enhancing resilience to external shocks.[11] I will elaborate on this remedy below.

Ultimately, where the U.N. Millennium Project differs most from *Learning from Reform* is in the extent of knowledge that it assumes we have and consequently in the degree of self-confidence exhibited by its authors. The U.N. Millennium Project is based on the view that we basically know enough to mount a bold, ambitious, and costly effort to eradicate world poverty. We have successfully identified all the margins that matter, and we better move on all of them simultaneously. *Learning from Reform*, by contrast, is an ode to humility. What we have learned, it says implicitly, is the folly of assuming that we know too much. We need to downplay grandiose claims, move cautiously, and concentrate our efforts where the payoffs seem the greatest.

6. *A Practical Agenda for Formulating Growth Strategies*

But what is the operational content of such a cautious, experimentalist approach? If we adopt the path recommended by *Learning from Reform*, can we say anything more than "different strokes for different folks" or avoid a nihilistic attitude where "everything goes"? *Learning from Reform* says little that is useful on this, but I think the answer is "yes" to both questions. Let me briefly outline here a way of thinking about growth strategies that avoids some of the obvious pitfalls.

[11] For an empirical analysis which emphasizes the role of external shocks (in interaction with weak institutions) as the culprit for growth collapses, see Rodrik (1999).

This approach consists of three sequential elements. First, we need to undertake a *diagnostic analysis* to figure out where the most significant constraints on economic growth are in a given setting. Second, we need creative and imaginative *policy design* to target the identified constraints appropriately. Third, we need to *institutionalize* the process of diagnosis and policy response to ensure that the economy remains dynamic and growth does not fizzle out.

6.1 *Step 1: Growth Diagnostics*

Policy reforms of the (Augmented) Washington Consensus type are ineffective because there is nothing that ensures that they are closely targeted on what may be the most important constraints blocking economic growth. The trick is to find those areas where reform will yield the greatest return. Otherwise, policymakers are condemned to a spray-gun approach: they shoot their reform gun on as many potential targets as possible, hoping that some will turn out to be the ones they are really after. A successful growth strategy, by contrast, begins by identifying the most binding constraints.

But can this be done? In Hausmann, Rodrik, and Velasco (2005), we develop a framework that we believe suggests a positive answer. We begin with a basic but powerful taxonomy (see figure 1). In a low-income economy, economic activity must be constrained by at least one of the following two factors: *either* the cost of finance must be too high *or* the private return to investment must be low. If the problem is with low private returns, that in turn must be due *either* to low economic (social) returns *or* to a large gap between social and private returns (low private appropriability). The first step in the diagnostic analysis is to figure out which of these conditions more accurately characterizes the economy in question.

Fortunately, it is possible to make progress because each of these syndromes throws out different sets of diagnostic signals or generate different patterns of comovements in

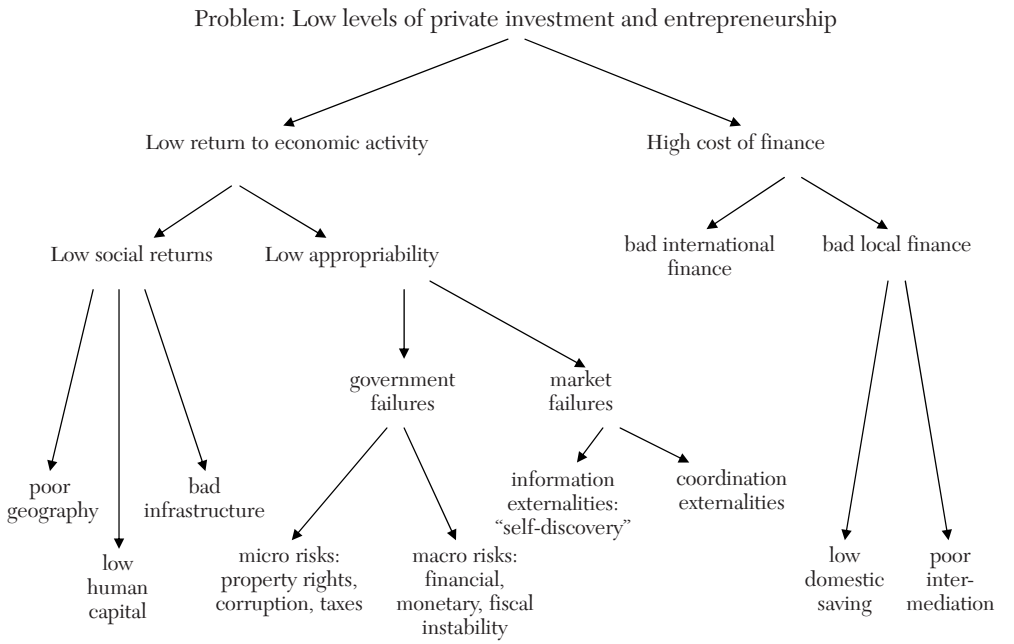

Figure 1. Growth Diagnostics

economic variables. For example, in an economy that is constrained by cost of finance we would expect real interest rates to be high, borrowers to be chasing lenders, the current account deficit to be as large as the foreign borrowing constraint will allow, and entrepreneurs to be full of investment ideas. In such an economy, an exogenous increase in investible funds, such as foreign aid and remittances, will spur primarily investment and other productive economic activities rather than consumption or investment in real estate. This description comes pretty close to capturing the situation of countries such as Brazil or Turkey, for example. By contrast, in an economy where economic activity is constrained by low private returns, interest rates will be low, banks will be flush in liquidity, lenders will be chasing after borrowers, the current account will be near balance or in surplus, and entrepreneurs will be more interested in putting their money in Miami or Geneva than in investing it at home. An increase in foreign aid or remittances will finance consumption, housing, or capital flight. These in turn are the circumstances that characterize countries such as El Salvador and Ethiopia.

When we identify low private returns as the culprit, we will next want to know whether the source is low social returns or low private appropriability of those returns. Low social returns can be due to poor human capital, lousy infrastructure, bad geography, or other similar reasons. Once again, we need to be on the lookout for diagnostic signals. If human capital (either because of low levels of education or the disease environment) is a serious constraint, we would expect the returns to education or the skill premium to be comparatively high. If infrastructure is the problem, we would observe the bottlenecks in transport or energy, private firms stepping in to supply the needed services, and so on.

Appropriability problems—i.e., a large gap between private and social returns—can in turn arise under two sets of circumstances. One possibility has to do with the

policy/institutional environment: taxes may be too high, property rights may be protected poorly, high inflation may generate macro risk, labor–capital conflicts may depress production incentives, and so on. Alternatively, the fault may lie with market failures such as technological spillovers, coordination failures, and problems of economic "self-discovery" (i.e., uncertainty about the underlying cost structure of the economy; see Hausmann and Rodrik 2003). As usual, we look for the tell-tale signs of each of these. Sometimes, the diagnostic analysis proceeds down a particular path not because of direct evidence but because the other paths have been ruled out.[12]

It is possible to carry out this kind of analysis at a much finer level of disaggregation, and indeed any real-world application has to be considerably more detailed than the one I have sketched here. But I hope this summary conveys the value of an explicitly diagnostic framework. Even a rudimentary application of these principles can sometimes reveal important gaps or shortcomings in traditional reform packages. For example, when the cost of finance is an important binding constraint (as seems likely in Brazil), institutional improvements aimed at improving the "business climate" (i.e., reducing red tape, lowering taxes, and so on) will be not only ineffective (since the problem does not lie with investment demand), but it can also backfire (since an increase in investment demand will put further upwards pressure on interest rates).

6.2 *Step 2: Policy Design*

Once the key problem(s) are identified, we need to think about the appropriate policy responses. The key in this step is to focus on the market failures and distortions associated with the constraint identified in the previous step. The principle of policy targeting offers a simple message: target the policy response as closely as possible on the source of the distortion. Hence if credit constraints are the main constraint, for example, and the problem is the result of lack of competition and large bank spreads, the appropriate response is to reduce impediments to competition in the banking sector.

Simple as it may be, this first-best logic often does not work, and indeed can be even counterproductive. The reason is that we are necessarily operating in a second-best environment, due to other distortions or administrative and political constraints. In designing policy, we have to be on the lookout for unforeseen complications and unexpected consequences. Let me return to an example from China. Formal ownership rights in China's township and village enterprises (TVEs) were vested not in private hands or in the central government, but in local governments (townships or villages). From the lens of first-best reform, these enterprises are problematic since, if our objective is to spur private investment and entrepreneurship, it would have been far preferable to institute private property rights (as Russia and other East European transition economies did). But the first-best logic is not helpful here because a private property system relies on an effective judiciary for the enforcement of property rights and contracts. In the absence of such a legal system, formal property rights are not worth much, as minority shareholders in Russia soon discovered to their chagrin. Until an effective judiciary is created, it may make more sense to make virtue out of necessity and force entrepreneurs into partnership with their most likely expropriators, the local state authorities. That is exactly what the TVEs did. Local governments were keen to ensure the prosperity of these enterprises as their equity stake generated revenues directly for them. In the environment characteristic of China, property rights were effectively more secure under

[12] So in the case of El Salvador we concluded that lack of self-discovery was an important and binding constraint in part because there was little evidence in favor of the other traditional explanations (Hausmann and Rodrik 2005).

direct local government ownership than they would likely have been under a private property-rights legal regime.

Such examples can be easily multiplied (Rodrik 2005a). As an additional illustration, consider the case of achieving integration with the world economy. Policymakers in countries such as South Korea and Taiwan in the early 1960s and China in the late 1970s had decided that enhancing their countries' participation in world markets was a key objective. For a western economist, the most direct route would have been to reduce or eliminate barriers to imports and foreign investment. Instead, these countries achieved the same ends (i.e., reduce the antitrade bias of their economic policies) through unconventional means. South Korea and Taiwan employed export targets and export subsidies for their firms. China carved out special economic zones where foreign investors had access to a free-trade regime. Policymakers chose these unconventional solutions presumably because they created fewer adjustment costs and put less stress on established social bargains.

6.3 *Step 3: Institutionalizing Reform*

The nature of the binding constraint will necessarily change over time. For example, schooling may not be a binding constraint initially, but as investment and entrepreneurship pick up, it will likely become one unless the quality and quantity of schools increase over time. In Hausmann, Rodrik, and Velasco (2005), we illustrate this issue using the example of the Dominican Republic. This country was able to spur growth with a number of sector-specific reforms that stimulated investment in tourism and maquilas. But it neglected making the institutional investments required to lend resilience and robustness to economic growth—especially in the area of financial market regulation and supervision. When September 11 led to the drying of tourist inflows, the country paid a big price. A Ponzi scheme that had developed in the banking sector collapsed, and cleaning up the mess cost the government 20 percentage points of GDP and led the economy into a downward spiral. It turned out that the economy had outgrown its weak institutional underpinnings. The same can be said of Indonesia, where the financial crisis of 1997–98 led to total economic and political collapse. It may yet turn out to be case also of China unless this country manages to strengthen the rule of law and enhance democratic participation.

What is needed to sustain growth? Two types of institutional reform seem to become critical over time. First, there is the need to maintain productive dynamism. Natural resource discoveries, garment exports from maquilas, or a free-trade agreement may spur growth for a limited of time. Policy needs to ensure that this momentum is maintained with ongoing diversification into new areas of tradables. Otherwise, growth simply fizzles out. What stands out in the performance of East Asian countries is their continued focus on the needs of the real economy and the ongoing encouragement of technology adoption and diversification.

The second area that needs attention is the strengthening of domestic institutions of conflict management. The most frequent cause for the collapse in growth is the inability to deal with the consequences of external shocks—i.e., terms of trade declines or reversals in capital flows. Endowing the economy with resilience against such shocks requires strengthening the rule of law, solidifying (or putting in place) democratic institutions, establishing participatory mechanisms, and erecting social safety nets. When such institutions are in place, the macroeconomic and other adjustments needed to deal with adverse shocks can be undertaken relatively smoothly. When they are not, the result is distributive conflict and economic collapse (Rodrik 1999). The contrasting experiences of South Korea and Indonesia in the immediate aftermath of the Asian financial crisis in 1997–98 are quite instructive in this regard.

Institutional reforms in these areas are difficult to implement and they take time. Economic science typically provides very little guidance on how to proceed (Avinash K. Dixit 2004). But the point is that these difficulties do not need to stand in the way of formulating less ambitious, more selective, and more carefully targeted policy initiatives that can have very powerful effects on igniting economic growth in the short run. What is required to *sustain* growth should not be confused with what is required to *initiate* it.

7. *Concluding Remarks*

It is now time for a confession. As the preceding discussion ought to have made clear, I find *Learning from Reform* a useful and important document in no small part because its central themes parallel those that I have been advocating for some time along with a number of my colleagues at the Kennedy School (see in particular Rodrik 2005a; Hausmann, Rodrik, and Velasco 2005; and Hausmann, Pritchett, and Rodrik 2005). It is gratifying to see one's ideas being taken seriously, particularly by an institution that has frequently served as a target for one's criticisms. The report pays me compliments in other ways too: one of its two opening quotes is taken from my work (the other is from Al Harberger). And I return the compliment by acting as one of the endorsers on its back cover.[13] Had the editor of this *Journal* not insisted, I would not have found it proper to write this review essay.

But I would like to think that the laudatory note I have struck above has to do not just with an ego that is being stroked. Coming from the institution that is one of the chief architects of the reforms of the last twenty years, *Learning from Reform* is a genuinely interesting document: it represents a mea culpa as well as a way forward. It pushes us to think harder and deeper about the economics of reform than anything else out there. It warns us to be skeptical of top-down, comprehensive, universal solutions—no matter how well intentioned they may be. And it reminds us that the requisite economic analysis—hard as it is, in the absence of specific blueprints—has to be done case by case.

These should be music to any economist's ears. After all, what distinguishes professional economists from ideologues is that the former are trained to make *contingent* statements: policy A is to be recommended only if conditions x, y, and z obtain.[14] Sensible advice consists of a well-articulated mapping from observed conditions onto its policy implications. This simple but fundamental principle seems to have gotten lost in much of the thinking on economic reform in the developing world, which has often taken an a priori and mechanical form. Its

[13] To add to the incestousness of the relationship, Lant Pritchett, my coauthor on Hausmann, Pritchett, and Rodrik (2005), served as the principal author of two of the chapters of *Learning from Reform*.

[14] As a trite, but still useful illustration, consider trade liberalization, which is one of the most common policy reforms recommended to developing countries (typically unconditionally) (Rodrik 2005a). Economic theory says that trade liberalization is guaranteed to enhance welfare only under a long list of conditions: The liberalization must be complete or else the reduction in import restrictions must take into account the potentially quite complicated structure of substitutability and complementarity across restricted commodities. There must be no microeconomic market imperfections other than the trade restrictions in question, or if there are some, the second-best interactions that are entailed must not be adverse. The home economy must be "small" in world markets or else the liberalization must not put the economy on the wrong side of the "optimum tariff." The economy must be in reasonably full employment or, if not, the monetary and fiscal authorities must have effective tools of demand management at their disposal. The income redistributive effects of the liberalization should not be judged undesirable by society at large or, if they are, there must be compensatory tax-transfer schemes with low enough excess burden. There must be no adverse effects on the fiscal balance or, if there are, there must be alternative and expedient ways of making up for the lost fiscal revenues. The liberalization must be politically sustainable and hence credible so that economic agents do not fear or anticipate a reversal. And an even longer list of requirements would have to be present for trade liberalization to generate *economic growth*, i.e., go beyond static Harberger triangles. While the theory of the second-best should not paralyze us, neither should we hand-wave it away as easily as we seem to do in our role as policy advisors.

rediscovery is therefore good news not just for poor nations, but for the economics profession as well.

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